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PARADIGMS AND EXEMPLARS IN SOCIOLOGY: A KUHNIAN REFORMULATION

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1. Introduction

It is now more than thirty years since Thomas Kuhn published his *Structure of Scientific Revolutions* (1962). But although recourse to the notion of paradigm has become something of a fad in the social sciences since Robert Friedrich's *Sociology of Sociology* (1970), with one exception¹, Kuhn's reappraisal of the nature of scientific practice has not yet been faithfully applied to the sociological enterprise.² The objective of this essay is to propose a concrete example of such an application and, at the same time, to reevaluate the usefulness of the Kuhnian approach in the sociology of sociology. The question is of importance, since the answer we choose to give it will determine our adherence either to an image of social science as a pluralistic adventure of the mind, or as a totalitarian enterprise reducing any attempt to think in different terms to a common frame.

In 1979, Douglas Eckberg and Lester Hill published a critical appraisal of the different attempts at application of Kuhn's views to sociology, and concluded that the results were "far from satisfactory" (1979, 925). That their criticisms were justified from a Kuhnian perspective has been recognized even by one of the main culprits designated in their paper: George Ritzer³. The present

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- 1 The only true alternative I am aware of is the one proposed by Colclough and Horan (1983). They analyze the "status attainment paradigm" in a perspective that is faithful to the Kuhnian tradition. But because they limit their analysis to this particular example, they tend to unduly restrict the import of Kuhn's work for the sociology of sociology.
 - 2 Hence the apparently somewhat paradoxical title of this essay: although the notions of paradigm and exemplar are generally considered Kuhnian by definition, the way they have been applied in sociology is very un-Kuhnian.
 - 3 In his comments on Eckberg and Hill's paper, Ritzer recognizes that "operating from their three basic assumptions, Eckberg and Hill are virtually unassailable in their contention that the paradigm concept has been misused by those who have attempted to apply it to sociology" (1981, 245). Ritzer's "three assumptions" are: 1) a focus on "the sociology of science, specifically Kuhnian theory"; 2) "being greatly concerned with being true to Kuhnian theory"; 3) "[preferring Kuhn's] later definition of a paradigm as an exemplar". Actually, the first assumption misrepresents Eckberg and Hill's position (Hill and Eckberg 1981), and the third is superfluous: if one accepts assumption 2), one is *thereby compelled to* "prefer" Kuhn's "later" definition.

analysis builds on the foundations provided by Eckberg and Hill's critique. To briefly summarize their arguments, sociologists have failed to take into account the fundamental innovation brought about by Kuhn's approach. One of the main innovations of this approach lies in its emphasis on the concrete technical problems encountered in everyday scientific activity (Kuhn 1971, 136). This preoccupation with scientific *practice* (rather than with abstract methodological rules; Masterman 1970, 60; Barnes 1982, 58–63) results in at least three major departures from standard history of science: 1) an accent on the cognitive mechanisms involved in the routine exercise of science; 2) an increased interest in the socialization of the would-be scientist; 3) a constant preoccupation with the social background in which these activities take place: the scientific community. These innovations all simply vanish when sociologists consider that a "paradigm" is a set of abstract theoretical, or meta-theoretical assumptions, which need not be shared by any concrete scholarly community. As has been shown by Eckberg and Hill, sociologists using this approach simply "slice up the sociological pie" (for ex. consensus vs. conflict theory, or social facts, definitions and behavior) as suggested to them by their personal preferences. As a result, at least twelve different paradigmatic structures for sociology have been proposed (1979, 929–930). But the problem is precisely that "one cannot divide a discipline freely into paradigms, but must be constrained by both the group structure and cognitive consensus" (1979, 933; see also Kuhn 1969a, 176).

The very existence of these failed attempts raises some doubts about the applicability of the Kuhnian model to sociology. As a matter of fact, Kuhn himself is rather sceptical regarding the possibility of applying his views to such "proto-sciences" as the social sciences.⁴ In his view, our discipline is in the same situation as physical optics before Newton (1959, 231) or, say, "chemistry and electricity before the mid-eighteenth century" (1970, 244).

Since the very inception of sociology, its practitioners have tended to angrily reject such pronouncements. They generally resent being considered outsiders to the scientific enterprise. Disregarding Kuhn's explicit cautions⁵, some of

4 However, in some places, Kuhn has expressed himself differently; for example: "Whatever paradigms may be, they are possessed by any scientific community, including the schools of the so-called pre-paradigm period. My failure to see that point clearly has helped make a paradigm seem a quasi-mystical entity or property which, like charisma, transforms those infected by it" (1976, 295, n. 4). The question whether the social sciences are pre-paradigmatic or not thus seems to remain open.

5 "If... some social scientists take from me the view that they can improve the status of their field by first legislating agreement on fundamentals and then turning to puzzle solving, they badly misconstrue my point" (Kuhn 1970, 245).

them have seized upon his scheme in an attempt to show that the social sciences, too, could be considered “mature sciences”. But ironically, it is the very urgency of the concern that lay at the origin of this attempt that explains its failure.⁶ Some sociologists have striven to show that the social sciences also display – at least potentially – the characteristics of a “normal science”. But a strange misreading of Kuhn has led them to conclude that the criterion for this achievement lay in a sort of *theoretical*, or even *meta-theoretical* unity (note, however, that this is not the case of Friedrichs)⁷. According to this view, the social sciences can become mature to the extent that they become unified under a single “paradigm”. But through this annexation of the Kuhnian terminology, sociologists have only dignified with a new name a very old ambition. This search for unity, which started with Comte, has been repeatedly taken up again and presented in more “acceptable” forms by legislators of sociology like Parsons and, more recently, Alexander (1982).

The same misreading is also frequent among philosophers of science, some of whom have accused Kuhn of propagating a totalitarian view of science. Reacting to Kuhn’s assertion that “it is precisely the abandonment of critical discourse that marks the transition to a science” (1965, 6), they have compared the Kuhnian scientific community with a “closed society” in the Popperian sense (Watkins 1965, 26), criticized his view as “[inhibiting] the advancement of knowledge [and increasing] the anti-humanitarian tendencies which are such a disquieting feature of much of post-Newtonian science” (Feyerabend 1970, 197–198), and concluded that “the ‘normal’ scientist, as Kuhn describes him, is a person one ought to be sorry for” (Popper 1965, 52).

If, in order to become a “mature science”, sociology indeed had to move “beyond criticism” to become entirely subordinated to a unitarian framework, then indeed Kuhn’s approach would be inherently conservative (Restivo 1983), and even anti-humanitarian. In this paper, I will argue that this is not the case, and that, if carefully read and correctly applied, Kuhn’s framework results not in the submission to a totalitarian framework, but precisely in liberation from the hegemonic frameworks to which meta-theoreticians like Alexander (1982) or Ritzer (1983, 512–518) would like to submit us by forcing the diversity of sociological approaches into neat but largely artificial “continuums”. Furthermore, it leads to the conclusion that the social sciences do not compare so badly with the natural sciences. Not because, as the interpretation mentioned

6 This is also the conclusion reached by Eckberg and Hill (1979, 933–934).

7 Friedrichs takes the position that it is the very *diversity* within sociology that constitutes proof of its maturity (1970, 2). He overlooks the fact that for Kuhn, *normal science*, and not revolutions, is a sign of maturity.

above would have it, they are approaching “paradigmatic unity”, but because, all things considered, the natural sciences are not so very different from the social sciences in their actual functioning.⁸

My attempt to apply the Kuhnian framework to sociology is not purely formal. I am not concerned with theoretical purity as such, but with showing that the innovations proposed by Kuhn can help us better understand our own discipline. I have become aware of this possibility while carrying out historical research on a very specific area of sociological theory: the secularization issue. The Kuhnian framework has helped me give a new and, I believe, more coherent interpretation of this issue (Tschannen *forthcoming*). In the following analysis, I will draw freely on this study for illustration.

In order to provide a faithful and coherent application of the Kuhnian framework to sociology, we will have to start from scratch, that is, from a direct analysis of Kuhn’s writings⁹, bypassing the misinterpretations that have accumulated over the last three decades.¹⁰ But first, we must rapidly dispel the shadow of these customary interpretations, which might prevent us from a correct understanding.

8 This assertion rests mainly on the tradition issued from the “strong programme” in the sociology of science (Bloor 1976), and the works of Latour and Woolgar (1979), but it also owes much to Kuhn’s influence. In his writings, one finds literally dozens of small notes that relativize the barrier between the natural and the social sciences. Thus for example, after remarking that, very often, scientists are unable to obtain certain numerical results as long as they do not know precisely what these results are supposed to be, Kuhn concludes: “There are self-fulfilling prophecies in the physical as well as in the social sciences” (1961, 196). More fundamentally, Kuhn’s view, like those of the proponents of the strong program, rests on the notion that “data” are the result of social interaction within a particular community (1974, 308–309).

9 I have read all of Kuhn’s writings that, judging from their title and from cross-references, appeared to bear on the theory of paradigms and exemplars. For a complete bibliography of Kuhn’s works up to 1986 (and an excellent and very detailed overview of the Kuhnian approach), see Hoyningen-Huene 1989.

10 As the main purpose of this study is not critical, but constructive, I will not provide a systematic overview of these positions. Most of those that had been formulated before 1979 have been discussed thoroughly and very competently by Eckberg and Hill (1979). In the intervening decade, although there seems to have been somewhat less enthusiasm for the kind of approach criticized in this paper, work in this direction has continued (for an overview, see Ritzer 1983 [second edition: 1988], 505–507). But as far as I know, no thorough reformulations have been attempted. There have been a few attempts to use the notion of exemplar (see for example Wagner and Berger 1985, Jones 1986). Unfortunately, these attempts are too superficial. Another earlier such attempt is Friedrich’s own discussion of an “exemplar” in sociology (1972). Although the perspective is, here again, unduly restricted to one very peculiar case, his approach in this particular paper (though not in his earlier book) is fairly faithful to Kuhn’s intention. The only true alternative I am aware of has been mentioned in the first footnote.

As Kuhn himself has recognized, he has used the term “paradigm” in at least two very different senses (1976, 294).¹¹ In none of these senses does it refer to a theory (Masterman 1970). The first sense of paradigm is meta-theoretical: Scientific practice ultimately rests on a set of values. For instance, the choice between different theories cannot always rest on rational criteria, because these criteria may be conflicting: One theory may be more accurate, while the other may have a larger scope (Kuhn 1973, 322). As has been remarked by Eckberg and Hill, sociologists have almost exclusively focused on paradigms as meta-theoretical assumptions.

The second sense of paradigm is infra-theoretical. This is the sense that is absolutely central to Kuhn’s approach¹², and which has curiously been ignored by most sociologists.¹³ We can illustrate this point by an analogy drawn by Kuhn in a discussion with historians of art. According to Kuhn, “if the notion of paradigm can be useful to the art historian, it will be pictures not styles that serve as paradigms” (1969b, 351). From now on, we will ignore styles, and concentrate on pictures. The gist of the following argument will turn around the infra-theoretical component of the term paradigm.

2. “Exemplars” in Kuhn’s work

The starting point for a correct understanding of Kuhn is cognitive. The question we must ask is: how do scientists learn their trade? The pre-Kuhnian answer to that question was: through the use of formal definitions and the reference to explicit criteria. Kuhn’s answer is very different, and relies on the notions of *ostension* and *exemplar*. When he learns his trade, the would-be scientist activates some elementary cognitive devices which are shared by all

11 I will not engage into the debate concerning the question of the “early” vs. the “later” Kuhn, but will consider his works as a coherent whole, disregarding the ambiguities contained in the earlier statements. Even if it seems to me that some of the statements to the effect that Kuhn has radically shifted his position in the course of the years are exaggerated, there clearly is a problem with Kuhn’s lack of consistency over time. Clearly, he who must too often resort to claiming that his opponents have completely misunderstood him (see for example Kuhn 1970, 231; 1976, 293) must take at least some of the blame on himself. In order to allow the reader to gain at least some historical perspective, I refer to Kuhn’s works (including several papers that have been reprinted in 1977) by the year of their first publication.

12 It is indeed this sense of the term that Kuhn had in mind when he started using it (Kuhn 1974, 318; 1977, xix).

13 Although Ritzer for example understands Kuhn’s notion of exemplar and knows that he considers it to constitute the core of his approach, he brushes these considerations aside with a sleight of hand: “In my opinion, the concept of metaphysical paradigm is far more useful in analyzing the status of any science than the concept of an exemplar” (1975, 5–6).

humans. To make his case, Kuhn takes as an illustration a small child who takes a walk with his father.

The child already recognizes birds in general, and certain specific kinds of birds, but he cannot yet recognize swans, geese, or ducks. To him, these are all simply “birds”. Now the father could try to define swans by giving an explicit set of criteria, one of which could be: “All swans are white”. But this would be very difficult. Unless he is an ornithologist, the father would be unable to provide such a set of reliable criteria. But fortunately, as Kuhn explains, “there is a means of processing data into similarity sets which does not depend on a prior answer to the question, similar with respect to what?” (1974, 308). What the father will do is simply point to the birds, and say: “Look, this is a swan; and this is a goose”. After a few such examples have been given, the child will himself start pointing to birds and designating them. If he makes a mistake, the father will correct him: “No, that’s a duck”. Fairly rapidly, the child will have learned to recognize the three kinds of birds. The whole learning process is achieved by *ostension*, and relies on the ability of the human mind to recognize similitudes and to group objects accordingly.¹⁴ Thus learning occurs without recourse to explicit definitions (Kuhn 1974, 312), indeed – in the case of science and, as we will see, in the case of sociology – in spite of the fact that no standard set of defining characteristics can be agreed upon.

This cognitive process based on the recognition of similitudes is what first led Kuhn to use the term paradigm. “That procedure seemed very close to the one by which students of language learn to conjugate verbs and to decline nouns and adjectives. They learn, for example, to recite *amo, amas, amat, amamus, amatis, amant*, and they then use that standard form to produce the present active tense of other first conjugation Latin verbs. The usual English word for the standard examples employed in language training is ‘paradigm’, and my extension of that term to standard scientific problems like the inclined plane and the conical pendulum did it no apparent violence” (Kuhn 1977, xix).

The same principles apply in the case of scientific education. Consider for example the way in which the student learns to recognize whether a given set of data resulting from an experiment can be considered consistent with a given theory or not. Given the fact that “perfect agreement” does not exist in practice, the student must acquire a sense of what can be considered “reasonable agreement” between the data and the figures predicted by the theory (Kuhn 1961, 184–185). But this cannot be achieved by the definition of a set of

14 Note also that, in a different culture, the child might have learned to group these birds in different clusters.

criteria, since no such “consistently applicable” criteria exist. Here, the textbook – a very important device in the Kuhnian framework – enters the stage: it will teach the student to recognize “reasonable agreement” by *ostension*. The textbook will display a recognized example of scientific achievement, and, simultaneously, the numeric distribution that is supposed to result from the experiment. The function of the table is *not to make an attempt at infirming the theory* (1961, 182). It is to *display* what is regarded in the scientific community as an example of an “acceptable” distribution for the related experiment. Of course, it is only after having been exposed to *several* such examples that the student will be able to recognize “reasonable agreement” at first glance (Kuhn 1979, 412–413).

Or consider another example, that of Newton’s Second Law of Motion, which stipulates that the force is equal to the mass, multiplied by the acceleration ($f=ma$).¹⁵ This formula cannot be applied directly in concrete scientific experiments, but functions as a “law-sketch” that can be applied analogically. “For the problem of free fall, $f=ma$ becomes $mg=md^2s/dt^2$. For the simple pendulum, it becomes $mg\sin\theta=-md^2s/dt^2$ ” (Kuhn 1974, 299). These formulas are *not* obtainable directly through mathematical transformations of $f=ma$. In learning to apply $f=ma$ in different situation under the guidance of his instructor, the student will learn to recognize similarities between different situations, in other words, he will learn to group different experiments under a general law-sketch ($f=ma$) on the basis of a number of similitudes, just as the child has learned to group different birds under the same name. Another closely related instance is that of the three different situations in which XVIIIth century mechanicians learned to apply the principle of *vis viva*: “Actual descent equals potential ascent” (Kuhn 1969a, 190–191). The three situations are the ball rolling down an inclined plane and then up a slope (Galileo), the pendulum (Huyghens), and the flow of water from an orifice (Bernouilli).

3. Exemplars in sociology

Let us now turn to sociology. In doing this, we will have to be very careful, and make sure that what we call an exemplar in sociology is really comparable to what Kuhn calls an exemplar in science. This rule is violated, for example, when Ritzer argues that, in the framework of his “social-facts paradigm”,

15 Actually, $f=ma$ is not exactly an exemplar, but the symbolic generalization of an exemplar (Kuhn 1974, 297–299). But this does not alter my argument, since the function of both devices is the same. Kuhn himself treats $f=ma$ as an exemplar in the “Postscript” to his *Scientific Revolution* (1969a, 187–191)

Durkheim's *Rules of Sociological Method* is an exemplar, while "Weber's work on social action" is the exemplar of the "social-definition paradigm" (Ritzer 1983, 510). Of course, in Kuhn's view, it is not "Newton's work", nor even his *Opticks*, that are taken as exemplars, but $f=ma$ or, more strictly speaking, experiments like that of the inclined plane.

Is it possible to find an equivalent for $f=ma$ in sociology¹⁶, and, if the answer is positive, what would this sociological equivalent of the Kuhnian exemplar be? As we will see, the answer is disappointingly trivial. Imagine an instructor in an introductory sociology course wants to present Weber's three types of authority.¹⁷ The initial situation is the same as those discussed previously: the instructor faces the task of teaching his students a way of distinguishing among different "objects" that have hitherto been considered by them under a single category: "authority in general". Of course, the instructor will rely on a number of formal definitions.¹⁸ But what he will be most likely to do next (like

16 Let us briefly note that Kuhn himself – who, for all his talk about sociology as a "proto-science", relies on it very much – at least once uses the notion of exemplar as a sociological analytic category, when he speaks of "the Enlightenment's vision of science as at once the source and the exemplar of progress" (1968, 106).

17 Of course, these are ideal-types, not objects given in nature. But so are swans – at least when viewed by a child taking a walk with his father, and not by an ornithologist applying the principles of taxonomy. No two swans are really alike, and their recognition by the child does not rest on explicit criteria, but on an intuitive capacity to recognize similarities. And it is precisely this capacity to "see" analogies which is relied upon in the construction of Weberian ideal-types.

18 However, when viewed closely, many sociological definitions read more like examples than like definitions. Witness Weber's own celebrated definitions of authority. To be short, I will quote only the definition of the first type. "The validity of the claims to legitimacy may be based on: 1. Rational grounds – resting on a belief in the legality of enacted rules and the right of those elevated to authority under such rules to issue commands (legal authority)" (Weber 1922, 215). Strictly speaking, the first part of the definition should suffice. Provided that we know what "rational" means, a definition of legal authority as based on rational grounds should be satisfactory. But of course, things are not quite so simple, for rationality, even (or especially) in Weber's works, can have different meanings. One could suppose, for example, that authority is "rational" if based on an objective consideration of the "professional competence" of the person claiming authority as a manager. In other words, authority could be considered rational to the extent that it is based on the pragmatic consideration that we must put in command those who have the best abilities to help the social group perform its task efficiently. But of course this is not what Weber means. Therefore, we face the same problem Kuhn pointed to in the natural sciences: we cannot give any uniformly applicable criterion to define rationality, therefore we must rely on examples. This, when we come to think of it, is what the rest of Weber's definition does. We can imagine a concrete individual (ourselves, or perhaps one of our acquaintances) believing in "the legality of enacted rules", and also another concrete individual (perhaps some official in our university) who has been "elevated to authority under such rules". But that is not all. The following thirty pages in Weber's text are devoted to a detailed analysis of his three types of authority. Whatever the analytical purpose of this discussion is, it also serves the function of providing the reader with literally dozens of examples.

Raymond Aron in his *Main Currents in Sociological Thought*), will be to propose a series of examples. Thus, to illustrate the exercise of rational domination, he might propose the tax-collector, the policeman, and indeed the whole administrative apparatus of modern society, including such functions as the regulation of auto traffic and final examinations in the university (Aron 1967, 236).

The same strategy is widely applied in all books of sociology, from introductory texts to the most advanced writings.¹⁹ Let us briefly mention an example drawn from the work of a sociologist who, probably better than anyone else, has succeeded in transmitting the sociological viewpoint to generations of students, Peter Berger. Speaking of ideology, Berger first provides a definition: "Sociologists speak of 'ideology' in discussing views that serve to rationalize the vested interests of some group" (1963, 41). Now, in spite of its clarity, this definition is not likely to make much sense for a beginner in the field. Berger therefore immediately provides some examples: "In this way, we can speak of 'ideology' when we analyze the belief of many American physicians that standards of health will decline if the fee-for-service method of payment is abolished, or the conviction of many undertakers that inexpensive funerals show lack of affection for the departed, or the definition of their activity by quizmasters on television as 'education'" (1963, 41). In the following lines, the insurance salesman, the burlesque stripper, the propagandist, and the hangman are also cited to flesh out the analogy and allow the student a sufficient number of instances to build up his notion of ideology. Similarly, in more standard textbooks, students are generally provided, not only with a glossary, but also with standard examples drawn from classic writings or from daily life. The texts are literally filled with boxes, tables, short stories, cartoons, and pictures (for a good example, see Goode 1984).

19 Of course, there are exceptions to this pattern. One of the most notable of them is provided by Talcott Parsons. But this exception in fact confirms the rule. In his most formal texts, Parsons hardly ever provides an example and relies almost entirely on formal definitions. The result is widely acknowledged as "superb unintelligibility". To make Parsons intelligible, all we have to do is translate him into the language of examples. Consider for example Mills' famous "translation" of the *Social System*. Parsons writes: "Attachment to common values means, motivationally, that the actors have common 'sentiments' in support of the value pattern, which may be defined as meaning that conformity with the relevant expectations is treated as a 'good thing' relatively independently of any specific instrumental 'advantage' to be gained from such conformity, e. g., in the avoidance of negative sanctions". Here is Mills' translation: "When people share the same values, they tend to behave in accordance with the way they expect one another to behave" (Mills 1959, 30 and 31). Surely, Parsons' version tells us more than Mills'. But after we have considered Mills' version, which is introduced as a concrete example drawn from daily life ("When people share..."), we can go back to Parsons' and much more easily make sense of his definition.

Note that the degree to which these concrete examples are actually *shared by the whole profession* is variable. Here (and not in the nature or function of these examples) lies probably one of the main differences between the natural and the social sciences. Although some examples are fairly well established in sociology (Think for example of Adam Smith's discussion of the manufacture of nails as the exemplar of the division of labor, of the Hawthorne experiments as the paradigm of the influence of human factors in management, of Goffman's example of the restaurant as the paradigm of the distinction between "front region" and "backstage", of the "wild child of Avernion" as the exemplar of the unsocialized human being, of Salom Asch's experiments on "visual judgment" as the exemplar of group influence, of Weber's use of Franklin's text as an exemplar of the spirit of capitalism, of IBM as the exemplar of a strong corporate culture or, among French sociologists at least, of the use of Crozier's study on the *Monopoly* as the exemplar of a "closed situation"), others are more open. But the *cognitive function* of example-based learning – which constitutes the core of the Kuhnian approach – is as central to sociology as to other academic disciplines.

Since sociologists rely on both formal definitions and examples in early socialization, my contention that this process relies chiefly on ostension might appear to be weakened. However, we should not overevaluate the importance of formal definitions in our own discipline. As we all know, total consensus around a definition never obtains, and the practitioner must always work with widely incompatible definitions. Consider, for example, the different approaches to "differentiation" used in different secularization theories. Bryan Wilson's approach is the most traditional. He views differentiation as a specialization of social roles and of the corresponding institutional spheres. "Whereas once the clergyman, if he was a diligent incumbent, was also the educator, the guardian of community morals, the social worker at times, even the magistrate, the sick visitor (when not actually a medical adviser), today, these roles have been taken over by others" (1976, 16). His account of differentiation does not depend on a theory: it is simply a description of something that has historically happened. Consider now Thomas Luckmann's approach. He explicitly seeks to distance himself from the traditional formulation of differentiation theory, as it has been expressed in functionalism (1967, 23). For him, differentiation is a phenomenon occurring mainly at the level of consciousness. It refers to the fact that "within a world view, a domain of meaning, which is specifically religious, has become differentiated" (1967, 56). Later in the process of secularization, as a result of this differentiation of the world view, social differentiation appears, in the form of a specialized "institutional basis" for this specific domain of meaning (1967, 63). In Luckmann's view, differentiation is

above all a process of fragmentation of the once homogeneous world view; the social structural aspects of this process are secondary. Consider now Richard Fenn's approach. In a sense, his approach is exactly the opposite of Luckmann's. He thinks that social differentiation arose first, and that cultural differentiation was a consequence of this initial differentiation (1978, 33). Even more significantly, Fenn's starting point is an explicit revolt against Parsons. Differentiation includes, not only a specialization of institutions, but, more interestingly and importantly, a differentiation and a separation of the levels considered by Parsons to be congruent:²⁰ culture, social structure, and personality (Fenn 1970, 131). In Fenn's theory, this results in a clash between different social definitions of the sacred, and in the "demand for clarification of the boundary between religious and secular issues" (1978, 32). In this view, the accent is shifted to the differentiation between the individual and the collective spheres, which is accompanied by a "dispersion of the sacred". To summarize: Wilson's notion of differentiation is limited to the social structure, Luckmann's is centered on the ideational realm, and Fenn's is so pluridimensional that it ends up splitting everything apart – structure, culture, and even structure *from* culture and the individual *from* the collective. We could introduce further complexity in the discussion by presenting the notion of differentiation as used by Robert Bellah (1963), by Parsons (1964), and by Niklas Luhmann (1977); but this should hardly be necessary to make the point, which is after all too painfully familiar.

The upshot of these considerations is the following. Even though it is doubtful whether many sociologists would be able to cite at will any particular definition of structural differentiation (or of almost any other sociological concept), all of them are able to use this, and most other notions current in the profession, in a quite competent manner. Indeed, only by forming and fleshing out their knowledge of sociological concepts through examples can sociologists sidestep the quandary of reconciling myriads of mutually incompatible definitions. As a result, in the social as well as in the natural sciences, it is never possible to fully understand a concept without taking into account the particular uses to which it has been put.²¹

Thus exemplars serve a fundamental function in the socialization process and in the practice of scientists as well as of sociologists. In science, the notion of exemplar has helped Kuhn account for the fact that, even in the absence of a

20 Parsons does recognize that a differentiation between these levels has occurred, but he maintains that they are nevertheless largely congruent.

21 This is, again, one of the fundamental axioms of the Kuhnian approach (Kuhn 1964, 258–259).

minimal set of shared rules, scientific communities are able to unproblematically conduct research. "Shared examples of successful practice [can] provide what the group [lacks] in rules" (Kuhn 1974, 318; see also 1962, 14). It is not hard to see that the same applies in sociology. Every author who constructs a new secularization theory displays a new example of successful practice by giving one more example of the ways in which a given concept may be used even in the absence of consensus on its definition. But we must go even further: not only are definitions not indispensable; they are generally *not useful* to the sociologist's task. No more, at least, than it is useful to define a swan as, say, a white bird: this could only pose problems the day we encountered a black swan (perhaps in Australia) and does not improve our working knowledge of "swans" (Kuhn 1965, 17–18; 1974, 316).

When we come to think of it, what we have shown thus far is not by any means surprising (nor should it have been unexpected). Why should it be surprising that sociologists operate with the same cognitive apparatus as children taking a stroll with their father, students grappling with physical equations, and mature scientists exercising their profession?²² However, this does not prove yet that there are paradigms in sociology in the Kuhnian sense. For in order to have a paradigm, we must have more than a collection of exemplars. First, the exemplars must allow us to *generate* and to *solve puzzles*. Second, this activity must take place within a *research tradition*. Third, this research tradition must be upheld by a concrete *scientific (or scholarly) community*. And fourth, the establishment of this tradition can take place only through a *scientific revolution*. Let us examine each of these four criteria in turn, starting with the puzzle-solving function, and see if sociology can live up to them.

4. The puzzle-solving function

Let us first consider an example of puzzle drawn from Kuhn's writings. The five planets known in antiquity all move eastward across the sky. But their motions are not uniform: at certain times, for a brief period, they reverse their course, and move westward. In the pre-Copernican cosmology, these irregularities were roughly accounted for by a complex combination of interlocked movements around the (immobile) earth. One of the devices to account

22 Imersheim (1977) has indeed argued that Kuhn's approach can be applied to everyday life. Let us also briefly note that this basic cognitive apparatus has not been uncovered only by Kuhn. As he himself remarks, his conception is very close to that developed in Wittgenstein's "family resemblances" (Kuhn 1962, 45; see also Imersheim 1977). It seems to me that it is also very close to Bourdieu's discussion of *habitus*.

for these irregularities was the combination of a deferent and an epicycle: one point A circled uniformly eastward around the earth (on the deferent), while the planet itself circled more closely around that same point A (on the epicycle). Seen from the center of the deferent (the earth), and given a proper ratio between the two movements, the planet thus appeared periodically to briefly reverse its course westward. (The same mechanism is used on some merry-go-rounds.) Other devices, most notably eccentrics and equants, the details of which need not concern us here, were also used to account for smaller irregularities. Now, these mechanisms produced only approximations to the actual observed movements of the planets. But the overall system performed very well indeed – so well that not even the system devised by Copernicus was more precise, nor inherently more convincing (Kuhn 1957, 169). As a result, during centuries, astronomers worked with this paradigm. Their efforts were never directed toward the establishment of a new system, but toward the concrete solution of the puzzles²³ constituted by the remaining unexplained irregularities. Their problem could be formulated as follows: “What particular combination of deferents, eccentrics, equants, and epicycles would account for the planetary motions with the greatest simplicity and precision?” (Kuhn 1957, 73). Now, note that exemplars both *constitute* and solve puzzles. For these puzzles would not have been puzzles in the first place if the previous explanations had not been produced. The precise irregularities that can be solved by a second epicycle do not become apparent as long as the first epicycle has not been put in place. More significantly, after the Copernican revolution had been completed by Kepler, these puzzles were replaced by completely new questions: “The geometrically simple and precise elliptical motions rather than the average drift now demanded explanation” (Kuhn 1957, 244–245).

Let us now consider an example drawn from sociology. The end of the 1960s witnessed the relatively sudden and unexpected emergence of a number of new religious movements (Robbins 1988). In the framework of the prevalent explanations concerning the fate of religion in the modern world, this emergence appeared like a sort of anomaly.²⁴ A number of secularization theories had asserted that religion had lost its power to shape social life. How was this new

23 Note that Kuhn himself does not use this term in the book from which this example is drawn, because he had not explicitly developed his paradigm framework yet.

24 Note however that, unlike in the previous example, the “cause” of the anomaly was a change in the “observed universe” (“reality”) rather than a modification in the “universe of the observer” (conceptual framework). This contrast is probably more related to the particular examples used in this passage than to the differences between astronomy and sociology as scientific disciplines.

situation to be seen; in what gestalt was it to be considered?²⁵ Did the new religious movements constitute a dramatic reversal of the age-long secularization process, or were they nothing but an insignificant resurgence? This question deserves to be called a puzzle. As in Kuhn's framework, the anomaly, which constitutes the puzzle, can only appear against the background of an established framework: without the expectations enshrined in the theories of secularization, the new religious movements would not have constituted an anomaly. The problem posed by the puzzle was a cognitive problem of the same sort as that of the child who must decide whether a new bird is a swan or a goose. Did the new religious movements belong to the *genus* "secularization" or not? After some hesitation, the great majority of sociologists decided that the new religious movements *did* belong to the phenomenon called "secularization"; more specifically, they were a "reaction", or a "response", to secularization (see the section on Wilson versus Stark and Bainbridge below). Actually, there was little else they could do. An anomaly can always be seen either as a puzzle (to be solved) or as a counter-instance (that indicates that the paradigm is no longer valid). But the latter solution is chosen only when a new, more convincing paradigm, is available, for "to reject one paradigm without simultaneously substituting another is to reject science itself" (Kuhn 1962, 79). No such alternative paradigm being available in sociology to explain the position of religion in the modern world, it is not surprising that new religious movements should not be regarded (except rhetorically) as counter-instances, but as a puzzle.

Now, we might ask, what was the exemplar involved in the solution of this puzzle? I would contend that the main exemplar involved was differentiation. According to the perception based on this exemplar, over the ages, religion as an institution had become progressively differentiated from the other main institutions: education, politics, science, and so forth. As a result, what happened in religion was no longer as significant for society at large as it used to be. But even more importantly, differentiation had led to an anomic society, in which a new need for community had arisen. This need could be fulfilled very well indeed by the new religious movements. In other words, the anomaly constituted by new religious movements was rapidly incorporated into the paradigm through a standard "mopping-up operation" in which differentiation functioned both as the cause of the emergence of new religious movements and as the reason for their insignificance.

25 The solution of puzzles is likened by Kuhn to the discovery of a new *gestalt* in a cognitively confusing environment: "Doing problems... resembles the child's puzzle in which one is asked to find the animal shapes or faces hidden in the drawing of shrubbery or clouds" (1974, 307).

Of course, the puzzles encountered by sociologists are not always as fascinating or as far-reaching in their consequences as this one. Although other cases of big puzzles in the sociology of religion could be cited²⁶, most puzzles are much smaller, hardly sticking out in the course of day-to-day routine. According to Kuhn, work in the framework of normal science consists “almost always [of] repetitions, with minor modifications, of problems that have been undertaken and partially resolved before” (1959, 233). As we all know, such activities occupy a very important place in day-to-day sociological practice. The great majority of papers published in sociology journals are very similar (in subject-matter, in methodology, and in their results) to a variety of papers published one, five, or fifteen years before.

Thus up to a point, sociological research is very similar indeed to research in science. One feature of puzzle-solving in sociology (at least in the particular area of secularization), however, is rather peculiar to the discipline. It seems that the solution of puzzles relies more on conceptual reformulations assisted by thought experiments than on the manipulation of hard data. But this characteristic is fully compatible with the Kuhnian model. Indeed, Kuhn himself insists on the fact that thought experiments *do* play a fundamental role in the natural sciences, and that they *do* teach us something about “the world” (1964). Another difference is that anomalies probably play a lesser role in sociology than in the sciences. The functional equivalent of anomalies might be boredom. Even when there are no obvious anomalies, social scientists sometimes have the feeling that their framework simply no longer allows them to *say anything interesting*.²⁷ This factor has played an important role in the revolution which led to the establishment of the secularization paradigm (see below): One only has to read the early *Acts* of the CISR (the scholarly community in which the revolution occurred) to notice the extent to which some practitioners were frustrated by their experiences because their results – even though they were accurate – did not lead them anywhere.

5. The research tradition

Let us now turn to the second issue. As mentioned above, the exemplars must be incorporated in a certain research tradition. In other words, what we are still

26 For example Herberg’s “American paradox” (Herberg 1955), Wuthnow’s attempt at explaining the irregularities in the secularization process (Wuthnow 1976), or Stark and Finke’s attack on Berger’s pluralism model (Stark and Finke 1988).

27 I owe this suggestion to Warren Hagstrom.

missing is the element of *consensus* over time. Of course, by its very nature, sociology cannot rest on a tradition as monolithic as that of the natural sciences. Still, in the case of secularization at least, the existence of a tradition can hardly be denied. Its continuity is not limited to the time of existence of the modern paradigm. The modern approach to secularization plunges its roots in a long tradition of sociological reflection on religion in the modern world. Many major sociologists and other thinkers have contributed to it. Thus Spencer, Durkheim, and Parsons have provided the tools for a better understanding of the place of religion in a differentiated society. Weber has provided us with a way of seeing that considers the present position of religion as the result of a process of rationalization in society at large, and that situates some of the roots of this process in religion itself. Theologians like Bonhoeffer, Bultmann, and Gogarten, whose views have been popularized by Cox (1965), have taught us that the apparent recession of religion can be viewed as a purification, and that secularization need therefore not amount to a decline of religion. On these sturdy conceptual foundations, modern theorists have built the first comprehensive models of the process of secularization. As a result of this major reformulation, the research tradition is also alive within the paradigm itself. Thus for example, a sociologist like David Martin (1978) could simply take the main contours of the process of secularization for granted, and directly work on refining this framework by analyzing the different outcomes of secularization as they are determined by the configuration of religious and secular forces within a given country. The analyses of “invisible religion” (Luckmann 1967) and of “civil religion” (Bellah 1967) have likewise spawned a substantial number of empirical reassessments and theoretical reformulations.²⁸

6. The scholarly community

Another important element is the scholarly community.²⁹ First, as has been underscored by Eckberg and Hill, it should be emphasized that the community which shares a paradigm is not the discipline as a whole (1979, 929). Although Kuhn recognizes that communities exist at different levels, one of which is the discipline as a whole, the type of community which interests him – and which is characterized by the fact that it shares common exemplars – is much smaller:

28 For a reassessment of Luckmann's theory, see for example Weigert 1974, and Machalek and Martin 1976. For Bellah's, see for example Jones and Richey 1974, and Gamoran 1990. The literature spawned by Bellah's approach is particularly impressive: the recently published *Index of Sociological Analysis* alone lists 15 papers under the heading of “civil religion”.

29 For an analysis of communities in sociology, see Mullins 1973.

Kuhn is convinced that such communities comprise “perhaps a hundred members, sometimes significantly fewer” (1974, 297).

In the case of secularization, evidence shows that this community exists, and is organized around the European *Société Internationale de Sociologie des Religions* (SISR – formerly CISR). Analysis of the literature, of the Acts of the CISR over a 40-year period, and interviews with 20 sociologists of religion related to the CISR, allow us to determine the shape of this community. It is organized around a core of two men: Bryan Wilson, a prestigious and influential figure in British sociology of religion, who is also one of the two most widely recognized and productive secularization theorists, and Karel Dobbelaere, the most successful systematizer of secularization theory (1981) and its best international expert. These two men entertain close professional and personal ties. Both have played a considerable part in the intellectual revolution we will describe shortly, and both have acted as presidents of the CISR. Very close to them, we find David Martin, also a former president of the CISR and close acquaintance of Wilson, who has written *A General Theory of Secularization* (1978). Somewhat more detached from this core, we find Richard Fenn and Thomas Luckmann. Both are members of the CISR and prominent secularization theorists. Next, we find a close friend of Luckmann, Peter Berger, who – although he is not a member of the CISR – has written the most famous secularization theory of all times: the *Sacred Canopy* (1967). Around these few sociologists, we find a close network of colleagues, friends, acquaintances, most of whom are members of the CISR. All of them are quite familiar with these theories and have commented upon them in print, or proposed alternative, if less successful, formulations.³⁰

7. The intellectual revolution

Finally, the genesis of a paradigm includes a *revolutionary* phase. According to Kuhn, “competition between segments of the scientific community is the only historical process that ever actually results in the rejection of one previously accepted theory and the adoption of another” (1962, 8). Normally, a scientific revolution is started by an internal mechanism (Kuhn 1971, 137–138). But it is

30 I am thinking in particular of Sabino Acquaviva, Jim Beckford, Jacques Billiet, Roberto Cipriani, Franco Ferrarotti, Antonio Grumelli, Gustavo Guizzardi, Danièle Hervieu-Léger, François-André Isambert, Christian Lalive d'Epinay, Jan Lauwers, Leo Layendecker, Frank Lechner, Meredith McGuire, Roland Robertson, Enrica Rosanna, Jean Séguy, Roy Wallis, and Keichii Yanagawa. Many more could be considered as belonging to this community although they have not written specific papers on secularization.

only natural that such a revolution should be more dependent upon factors external to the scientific logic in a discipline like sociology, which is less fully isolated from its environment (Kuhn 1957, 132; 1968, 118–119; 1977, xv). And indeed, in the case of the CISR secularization paradigm, the revolutionary phase – which included the arrival of a new generation of scholars within the CISR and the rapid accession of that new generation to positions of responsibility – included a certain amount of social conflict. This conflict expressed itself in clashes between an older generation of researchers, most of whom were churchmen, and who had been formed in the Catholic tradition of *sociologie religieuse* that had originated with Gabriel Le Bras, and a younger generation, most of whom were lay university professors interested above all in sociology rather than in the problems of the Catholic Church (Tschannen 1990).

8. The secularization paradigm

As we can see, sociology is able to live up to the four criteria defined in Kuhn's work for admission to "normal science".³¹ But it is probably still not clear what exactly it is that I am calling the "secularization paradigm". To understand this, a reversal of perspective is required: Different secularization theories must not be seen as "grounded in different paradigms", as Dobbelaere (1984), unfortunately following Ritzer (1975), has argued. Instead, we must consider that "*secularization*" itself is a paradigm. We will thus also conform to the criterion defined in the concluding section of Eckberg and Hill's paper: "If [paradigms] exist, they will be found in such areas as political socialization, status attainment, ethnic relations, and so forth, not in functionalism, conflict theory, and symbolic interactionism" (1979, 935). The paradigm presented in the following lines belongs to an area which might seem an unlikely candidate – the sociology of religion – and is concerned with the specific problem of secularization.

The secularization paradigm (Tschannen 1991) contains both levels of abstraction encompassed under the term "paradigm" in Kuhn's 1962 *Structure*. At the supra-theoretical level, secularization theorists indeed share a number of philosophic and methodological assumptions. For example, all their theories are compatible with the assumption that humans are simultaneously and

31 Obviously, nothing proves that we would discover similar patterns in other branches of sociology. The question of the extent to which these findings can be generalized must therefore remain open.

inseparably rational and symbolic (or religious) beings, and that religion will therefore never completely disappear. They also share an esthetic predilection for broad historical generalizations rather than painstaking empirical investigations. But most of these assumptions are simply consonant with those of the community of sociologists of religion at large. The infra-theoretical level is much more specific to the paradigm. It consists of a collection of exemplars, which are related in numerous ways.

We can now come back to the starting point of this paper. I had endeavored to show that, if correctly applied, the Kuhnian approach could allow us to escape from the totalitarian frameworks. What is in effect particularly interesting in the case we are discussing is that secularization as a paradigm encompasses into a coherent framework a number of formulations that are incompatible at the strictly theoretical level.

Thus for example, different secularization theorists work with very different theories of religion, and their reconstructions of the process of secularization do not overlap. Wilson defines religion in narrowly institutional terms (1966, xiii; xviii), and considers that the beginning of secularization coincides roughly with the scientific revolution. Berger's definition of religion has shifted over time (Berger 1974), but at the time he produced his secularization theory, it was much broader than Wilson's, being based on a phenomenological approach. Also, his process of secularization starts much earlier than Wilson's, in Ancient Judaism (1967, 115–121). Luckmann's definition of religion is even broader than Berger's, for it includes all the processes (for example socialization) that allow human organisms to transcend their biological nature and become selves (1967, 41–49). And his process of secularization starts so far back in time that it lies outside history altogether. Finally, Fenn refuses to define religion, and considers instead that the process of secularization *itself* is a constant struggle among social actors over the definition of the boundaries of the sacred (1978, 28–29). In this view, the process of secularization starts with the appearance of the first specialized priesthood.

In spite of these wide theoretical divergences, these authors share almost all of a series of exemplars which can be shown to be roughly organized around a core of three elements: differentiation, rationalization, and worldliness. The differentiation exemplar is particularly rich, since it is logically superordinated to a number of related sub-exemplars. For example, the privatization of religion and its generalization (for example as a "civil religion", or an "invisible religion") are two complementary movements arising out of the differentiation of the religious institutions. Other elements, like the autonomization of society, the decline of practice, the collapse of the world view, and pluralism, are logically related to differentiation.

These elements are shared even by sociologists who claim to have very severe disagreements, like for example Wilson on the one hand, and Stark and Bainbridge on the other hand. Very briefly, although Stark and Bainbridge claim that the advent of new religious movements disproves the “secularization thesis”, their own *Theory of Religion* (1987) proposes a view that is very close indeed to standard secularization theory.³² The disagreement between Wilson and Stark and Bainbridge is mostly a squabble over words (Tschannen *forthcoming*), as is exemplified by the fact that the first calls new religious movements a “response” to secularization (1975, 82), whereas the others prefer to call it a “reaction” (1985, 437). In fact, both frameworks rely largely on the same paradigmatic structure, and in both cases, new religious movements are a product of secularization, and the very existence of this product signals both the pervasiveness and the limits of secularization.

This reconstruction of the paradigm as a two-storied structure – which is fully authorized by Kuhn’s remark to the effect that the paradigm as exemplar is a “sub-set” of the paradigm as “all the shared commitments of a scientific group” (1974, 294) – also allows us to understand better how the transition from one paradigm to the next occurs. Kuhn’s notion of “gestalt-switch” (Kuhn 1962, 111–135) in effect presents some problems: according to this view, the transition from one paradigm to the next should be instantaneous (Watkins 1965, 35–37; Kuhn 1962, 150). In fact, we know that this is not the case. The solution of the riddle seems to be the following. The “conversion” to a new exemplar is indeed instantaneous. But the acceptance of the whole paradigm, with its host of exemplars and its articulation between the supra- and infra-theoretical levels, cannot be so quick: it must have a history, and can be completed only once the majority of the members of the scientific (or scholarly) community have been “converted” to a majority of the exemplars contained in the paradigm. (In the recent introduction to a book devoted to an analysis of his writings, Kuhn explains that he has recently realized that “groups do not have experiences except insofar as all their members do” [Kuhn 1989, 2], and that one could not simply consider that a group went through a gestalt-switch.) This is certainly what has happened in the case of the secularization issue, and, according to Kuhn’s own account, it is also what has happened in the case of the Copernican revolution. In the slow genesis of the new paradigm, we witness the invention of several fundamental exemplars, for example the notion that earth tends to fall, not toward the center of the universe (as Aristotle would

32 Of course, here again, there are very wide divergences among the theories themselves: Wilson’s is largely based on standard sociology and historical analysis, Stark and Bainbridge’s is much more formal, reductionistic, and positivistic. But the theses as to the mechanisms underlying secularization and the descriptions of the process itself are remarkably similar.

have it), but toward “other nearby bits of earth” (Kuhn 1957, 116), and the “impetus theory of motion” (an early prototype of Newton’s Law of Inertia, based on the observation that a projectile does not immediately fall to the ground when thrown but follows its initial impetus for some time; 1957, 119–20). As Kuhn himself concludes: “Major upheavals in the fundamental concepts of a science occur by degrees” (1957, 183; see also 1962, 7).

9. Conclusion

By showing that sociology could be considered to contain exemplars in the Kuhnian sense, I have not attempted to show that sociology *is* a science. The only thing I have tried to show is that it would be wrong to consider that it is *not* a science because it lacks theoretical unity. The Search for Unity can be abandoned without leaving the domain of science altogether: sociology can, and must, remain a pluralistic, critical, and self-critical discipline. Furthermore, it appears that, in applying the Kuhnian approach to sociology, we must not concentrate on discipline-wide analyses, but instead focus on studies of sub-disciplines and of the scholarly communities that constitute the social bases for these disciplines. The usefulness of the Kuhnian approach lies in the light it sheds at this more modest level of analysis.

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